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Interactive comment on "How sensitive are modeled contemporary subsea permafrost thaw and thickness of the methane clathrates stability zone in Eurasian Arctic to assumptions on Pleistocene glacial cycles?" *by* Valentina V. Malakhova and Alexey V. Eliseev

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article

We are grateful for the reviewer for constructive comments which led to the improved presentation of our results.

The most important changes in the manuscript are as follows:

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- The paper title is changed to 'The stability of contemporary subsea permafrost and associated methane hydrates to Pleistocene glacial cycles'. This is done in order to shorten the title and emphasise that we study the hydrates which are formed during the Pleistocene cold epochs.
- We revised the abstract to the paper as well as sections *Introduction* and *Conclusions* to highlight our motivation to undertake this study. In particular, we show that, while it is widely acknowledged that the response of the shelf sediments to imposed oceanic warming is a slow process, the time scale of such response is not yet quantified. Some references are added with respect to this discussion. We show that this time scale is of the order of 10-20 kyr for the deep present–day shelf, which is as twice as large in comparison to similar estimates obtained by Romanovskii et al. (2005).
- To highlight our results for the time scales of the response of the sediment thermal state to temperature changes at the ocean–sediment interface, we extended our paper by new Fig. 2, which shows the lag of the HSZ thickness D_{HSZ} with respect to T_B, and by the paragraph devoted to the discussion of this Figure. Previous Fig. 2 is now referred to as Fig. 3.
- We extended our paper by supplementary Fig. S2, which shows the results of additional simulations in which impacts of the pressure changes due sea level change are neglected.
- The language is checked and ameliorated.
- In addition, we discovered and corrected a technical error for our output for $z_{\mathrm{HSZ,t}}$ (Figs. 1, 3, and S1). This error does not affect the major outcome of our manuscript.

Below, the point-to-point replies to the comments are listed. Original reviewer's comments are typed in italic.

General comments

I am agree with Referee 1 that main deficiency of the paper is that the aim of the study, as well as main goal aren't formulated accurately. Also in the introduction one can't find exact formulation of what is known and what is not known about the subject under consideration.

- In the abstract, as well as in *Introduction* we highlight our motivation to undertake this study. In particular, we show that, while it is widely acknowledged that the response of the shelf sediments to imposed oceanic warming is a slow process, the time scale of such response is not yet quantified. In most previous papers the length of the performed simulations is up to few millennia which is not sufficient for such quantification (some references are added with respect to this discussion). The only exception, which we aware of, is the paper (Romanovskii et al., 2005) who also performed the simulations covering the whole glacial cycle. They obtained the time scale of the response of the subsea permafrost and of the subsea hydrates developed in this permafrost, which is of the order of 5–10 kyr.
- In sect. 2, we show that this time scale is of the order of 10–20 kyr for the deep present-day shelf, which is as twice as large in comparison to similar estimates obtained by Romanovskii et al. (2005). The likely reason of the latter difference are site–specific, non–monotonic profiles of the sediments thermophysical properties employed in the Romanovskii et al.'s paper. This, in principle, may diminish the generality of the conclusions of that paper.

Specific comments

One of main conclusions in the paper is that for HB not larger than tens meters temper-

ature change is main driver for the changes of HSZ boundaries, while pressure change is crucial for deeper HB. This conclusion seems improbable. For example, at 600 meters increase of pressure by 10 atm (100 meters of water column) should produce the same effect as decrease of temperature by approximately 2 K according to curve of methane hydrate stability. But figure 1e shows that temperature change at 600m is as large as 5 degrees and should produce larger effect. At 300 meters, increase of pressure by 10 atm produce the same effect as cooling by 4 degrees, but fig.1d show cooling by 5-10 degrees. The seeming coincidence of maximum HSZ extension and maximum sea level during interglacials shown in fig.1f can be explained by delay of cooling wave with increase in depth. So, categorical statement that increase of pressure rather than cooling is a primary source of increase of HSZ volume for deep HB should be removed from abstract, conclusion and the end of chapter 2. It would be useful if authors present figure similar to their Fig.1f (and may be 1d, i.e. for HB=50m) but for experiment with prescribed change of pressure only with surface temperature fixed at -1.8C during 400 kyrs.

We are very grateful for the reviewer for this comment. We agree that pressure changes can not cause onsets and disappearances of HSZ in our simulations. Only temperature changes may cause these onsets and disappearances. However, when the pressure and temperature at a given location in the the subsea sediments is close to a point at the HSZ stability curve, pressure changes may be important as well.

In particular, we made simulations similar to those presented in Sect. 2 of the manuscript, but imposing only temperature changes at the sediment-ocean water interface (accounting for oceanic transgressions and regressions) and neglecting changes of pressure due to sea level variations accompanying these transgressions and regressions. These simulations are alternative to the simulations suggested by the referee. We choose to perform the alternative simulations rather than those suggested by the referee because of the dominating impact of the temperature changes on the dynamics of subsea permafrost and HSZ: in the simulations suggested by the referee, subsea permafrost and HSZ would not form at all.

In these simulations, for intermediate and deep HB, the timings of the HSZ onset are very close to their counterparts on the original simulations. The same is true for the HSZ bottom changes during the initial stage of its shoaling. Nevertheless, in the original simulations, HSZ survives much longer than in the simulations with neglected impact of pressure variations on HSZ. In particular, for $H_{\mathrm{B}} = 100$ m, HSZ exists until ≈74 kyr B.P. in the original simulations and only until ≈106 kyr B.P. in the additional ones.

In response to this comment, we extended our manuscript by the respective discussion in the end of Sect. 2 and by the supplementary Fig. S2 in which the results of the just mentioned simulations are shown. The statement in the text, which are listed by the referee, are clarified.

Minor comments

- *P.2, line 25-32. Why TB =-1.8C is not the same as Tf=-1C? This point should be explained.*
  It was an erroneous statement right before the phrase indicated by the referee that in our model the salinity of water in sediment pores is set equal to the oceanic one. Upon revision, this statement is corrected. Now we state that our model lacks the module for calculating the salinity of pore water, and the chosen $T_{\mathrm{f}} = -1^{o}$C value is one of typical values used in other simulations (Nicolsky et al., 2012; Portnov et al., 2014). In turn, the value of $T_{\mathrm{B}} = -1.8^{o}$C was adopted from (Romanovskii et al., 2005; Razumov et al., 2014). The corresponding references are added to the body of the text. We note that there is a scatter of these values among different papers. For instanse, Portnov et al. (2014) use $T_{\mathrm{B}} = -0.5^{o}$C, but only for a small area near the Yamal Peninsula. In turn, the value of $T_{\mathrm{f}} = -2^{o}$C

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was used by Romanovskii et al. (2005). However, the precise values of T_B and T_f are of minor importance for the qualitative conclusions of our paper.
In addition, owing to T_B is always smaller than T_f in our simulations, a thin frozen layer survives in the upper part of the sediments even during interglacials. We acknowledge, that this may not be realistic. However, it hardly affects the main body of our results because we do not model the release of methane from hydrates. The respective discussion is added to Sect. 4.

- *P.2, line 32. Coefficient for specified initial linear temperature profile in K/m should be presented in the paper*
  It was an errouneous statement that the initial temperature profile in the sediments is linear. In the model, it is calculated as

$$T\left(z_j, t=0\right) = T\left(z_{j-1}, t=0\right) + G\left(z_j - z_{j-1}\right)/\kappa_j,$$

where κ_j is a heat diffusivity which is equal either to $\kappa_\mathrm{f} = 2.2$ W m^{-1} K^{-1} if a given computational level in the sediments is frozen or to $\kappa_\mathrm{u} = 2.0$ W m^{-1} K^{-1} if it is unfrozen. The subscript j indicates the computational level within the sediment (numbered from top to bottom), and $T\left(z_0, t=0\right) = T_\mathrm{B}(t=0)$. The resulting $T(z, t=0)$ profile is close–to–linear with respect to the vertical coordinate with the coefficient which is either 2.7×10^{-4} K m^{-1} or 3.0×10^{-4} K m^{-1} depending on the state of the sediments.